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PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

1841-42. No. 20.

Monday, 17*th January* 1842.

Dr ABERCROMBIE, V. P. in the Chair.

The following communications were read :—

1. On the Identity of the Animal Matters which form the Basis of the Animal Fluids and Solids. By James Stark, M.D., F.R.C. Phys.

The author, after referring to the late researches of Mulder, Vogel, Liebig, &c., whose experiments proved the close analogy between albumen, fibrin, and casein, and their almost identity in composition, stated that, in consequence of some experiments he had performed a few years ago, he was led to entertain the idea that all the animal fluids and solids consist of modifications of one and the same prototype animal matter. The following considerations seemed to confirm this opinion.

1. Experiments on the blood, and the fact of all tissues of the body, however differing in appearance and chemical properties, being formed from that albuminous fluid. 2. The formation of the chick in ovo, albumen and oil being the only matters from which all the tissues and fluids are derived; whilst the chick being shut up in a calcareous covering and tough membrane, which becomes denser as incubation advances, would appear to prevent any of the usual

supposed changes being produced, which could allow albumen to be converted into gelatin. 3. The continuity of tendon and muscle. The author stated, that if a fine fibre of tendon be carefully removed, it brings away with it its continuation in the muscle, or *vice versa*, and that, when this was examined by means of a powerful microscope, the continuity of the ultimate fibres appeared to him to be capable of being traced. 4. The mode in which a ruptured tendon is repaired,—the fibrinous matter which is thrown out, apparently by simple condensation, seeming to assume all the characters of true tendon. 5. The early food of the young of all the Mammalia, consisting, like that of the chick in ovo, of a kind of albuminous matter and oil, viz. milk. 6. Experiments on the nutrition of animals, which shew, that though fed with gelatin, fibrin, and albumen, the lacteals contain a simply albuminous fluid. 7. The similar effects produced by maçeration on all the tissues of the body.

The author then briefly narrated the chemical details, which satisfied him that all the solids and fluids of the body (fatty and nervous matters excepted), might be reduced to a prototype or basic animal matter, which exhibited none of the characters naturally belonging to any of the fluids of the body; and he shewed how the albumen in the serum of the blood seemed to be instantly resolved into this basic animal matter by a few drops of solution of caustic potash. He also found that the potash solution had a similar action on all the constituents of the blood, the fibrine as well as the albuminous matters not only preventing the blood from clotting but also from depositing its red or coloured particles. The author pointed out the effect of ether in removing this basic animal matter in a state of purity from its chemical solutions, and remarked that it acted similarly on solutions of albumen and of gelatin. He also stated, that this prototype animal matter appeared, in certain states of the system, to be secreted in its basic form along with the urine, and had been formerly described by him under the name of Gravidine.

The author therefore proposed to adopt from Mulder, for this basic animal matter, the name of *Protein*, as expressing the Proteus-like forms it is capable of assuming.

The author next proceeded to observe, that being desirous of ascertaining what effect chemical agents exerted on the tissues of the body in producing their solution, he was induced to examine the solutions by means of a powerful microscope, with a magnifying power of 800 diameters. He then observed that these were composed of an infinite number of minute globular particles floating in the clear fluid. These globules were many times smaller than the globules of the blood, and from their very minute size the author was only able to guess at their diameter. These globules, in the author's opinion, are not to be confounded with those described by Edwards, Baer, Sir E. Home, and others, who also stated the tissues to be composed of globular parts ; because the globules of these observers were of the same size as the blood corpuscles. These minute globules the author regarded as the ultimate particles of animal organization ; and though they seemed to consist of an external coat, and an enclosed transparent fluid, he thinks this is a deceptive appearance, and that it is more likely the particles are solid, as they apparently remain unchanged in size or form on drying. When the animal matters were removed from their chemical solutions in a state of purity by means of ether, and again examined, the author stated that these minute globular particles were even more distinctly seen than before, and that they retained their original globular appearance and general character apparently unchanged. He also stated that the blood to which potash had been added, was wholly reduced to its component globular particles floating either singly or in small irregular agglomerations ; but not a single blood corpuscle was visible ; all had been apparently resolved into thin constituent globular particles.

The author then stated, that he was induced to extend his examinations to the natural solids and fluids of the body, in order to ascertain whether any similar globular appearance could be detected in them. On a careful examination, almost all the tissues seemed to him to be composed of filaments of extreme tenuity, of the very same diameter as the minute globules he had previously observed in the chemical solutions ; and these filaments could, in many instances, be seen to be formed of globules or molecules arranged in lines. When the fibre or membrane was stretched, the globular or molecular appearance of the filaments appeared to be destroyed, but, when relaxed, the peculiar appearance became again distinctly visible.

On examining the fluids of the animal body, the author remarked that the fluid fibrin which rises to the top of new-drawn blood was composed of a multitude of minute globules of the same size and appearance as those previously seen in the tissues and in the chemical solutions. He further conceives, from his microscopic examinations, that the corpuscles of the blood are very compound bodies, composed of the same minute globules arranged in concentric rings. The serum of the blood of the lower animals also seemed to him to be filled with minute very transparent globules, but those in human serum were not always so easily detected. The pus and mucus corpuscles were also described as consisting of agglomerations of similar minute globules. The casein globules were also described.

The author stated that his observations had been made with a very excellent common compound microscope, but that he had verified every observation by means of crystal lenses of high power, simple globules of crystal, a garnet lens, with a magnifying power of 1000 diameters, and a Wollaston's doublet, adapted to the microscope.

2. On the Parasitic Fungi found growing on the Bodies of Living Animals. By John Hughes Bennett, M.D. Communicated by Dr Graham. Part 1. *See Proceedings of the next Meeting.*

The following Donations were presented to the Society since last Meeting.

Ordnance Survey of the County of Wexford in Ireland. 56 sheets. —*By his Excellency the Lord Lieutenant.*

Proceedings of the Linnean Society of London. Nos. 10, 11, and 12.

Transactions of the Linnean Society of London. Vol. xviii. part 4. —*By the Society.*

Monday, 7th February.

Sir T. M. BRISBANE, Bart., President, in the Chair.

The following communications were read :—

1. On the Parasitic Fungi found growing on the Bodies of Living Animals. By John Hughes Bennett, M. D. Communicated by Dr Graham. Part 2.

The author's object was, 1*st*, To confirm and extend the investigations of Gruby concerning the mycodermatous vegetations growing in the crusts of the cutaneous disease called *Porrigo lupinosa* ;—2*d*, To describe a plant he had himself found growing on the living membrane and tubercular matter of *Phthisis pulmonalis* ;—3*d*, To describe the structure of a plant found infesting the skin and gills of the gold-fish ;—and, 4*th*, To inquire into the pathological state which furnishes the conditions necessary for the growth of fungi in living animals.

1. In July last, the author examined the crusts of *Porrigo lupinosa*, readily detected the cylindrical and ramified appearances described by Gruby, and confirmed his account of the development of the disease. In some cases, the appearance of the crusts containing the vegetations was preceded by pustules; but as nothing of the kind would be observed in other instances, this seems an accidental, not an essential character of the disorder. The crusts appear in seven or eight days, in the form of minute, yellow, depressed spots, consisting of a smooth, cup-shaped capsule. This capsule, as seen with a magnifying power of 300 diameters, gives off from an amorphous granular matter numerous jointed tubes, which discharge from their extremities many round or oval bodies, the sporules or prolific germs of the plant. The appearance of the Porrigo-capsule was always preceded by desquamation of the cuticle. Hence it is probable that the sporules insinuate themselves into the crevices, instead of springing up originally below the cuticle, or in its substance, as Gruby supposes. The author was not more successful than Gruby, in communicating the disease by inoculation ; but nevertheless considers that the doctrine of its contagious nature, generally held by medical men, is sufficiently proved by other evidence. He thinks it is not confined to the human race only, as he has lately detected precisely the same appearances on the face of a common house-mouse.

2. While investigating the nature of pulmonary tubercles, the author observed a vegetable structure in the sputa expectorated during life, as well as in the tubercular matter found in the lungs after death, in a case of phthisis with pneumothorax. The structure consisted of jointed tubes branching dichotomously, between $\frac{1}{100}$ and $\frac{1}{200}$ of a millimetre in diameter, and springing from an amorphous granular mass. They gave off round or oval corpuscles, which appeared to be sporules, arranged in bead-like rows. These vegetations seemed to have existed during life, both because they were seen in fresh sputa, and because those seen in the lungs could scarcely have attained their actual development in the short space of thirty-six hours, which elapsed between the patient's death and the examination of the body. The plants resembled Link's *Penicillium glaucum*.

3. The gold-fish is known to be subject to a disease, accompanied

with the formation of a cottony substance around its gills, and on other parts of its body. In a specimen put into his possession by Mr Goodsir, the author found that the cottony substance presented under the microscope both a cellular and a non-cellular structure. The former consisted of long tubes divided into elongated cells, at the proximal end of which there was a transparent vesicle or nucleus about a 100th of a millimetre in diameter. Some cells were filled with a granular matter; others were empty, as if they had discharged their contents. Besides this structure, there were also long filaments, about a 600th of a millimetre in diameter, which sprung from the tubes, and seemed to consist of a diaphanous sheath, and a solid transparent matter. This structure, as in the two previous instances, sprung from a finely granular amorphous mass.

The author next gave a condensed view of the history of his subject, describing more especially the observations and discoveries of Bassi, Audouin, and Johanys relative to the Muscardine, or fungous disease, of silk-worms,—those of Hannover and Stilling, on the Confervæ which infest reptiles,—those of Ehrenberg, Goodsir, and Cooper, on the mycodermatous vegetables of fishes,—those of Owen and Deslongchamps, on birds,—and those of Schœnlein, Fuchs, and Langenbeck, Gruby, and others, on man. And from all the facts hitherto collected on the subject, he inferred, that the vegetations in question are not the cause, but the result of disease in animals,—that they grow on unorganized matters, apparently albuminous or tubercular in nature, which are effused into the healthy textures,—that they occur only in animals previously weakened by circumstances inducing imperfect nutrition,—and that their growth is to be counteracted partly by invigorating the body and partly by local applications hostile to vegetable life.

The paper was accompanied with drawings of the appearances described.

2. On the Action of Water on Lead. By Dr Christison.

The author, after briefly stating the results of his Experimental Inquiries, published on this subject in 1829, proceeded to describe two instances which had recently come under his notice, illustrative of the solvent action of certain terrestrial waters on lead, and of the danger of using this metal for conducting water in pipes, unless with a due regard to the circumstances which promote or prevent its corroding property. In one instance, the water of a spring, conveyed in a lead-pipe from a distance of three quarters of a mile, was found to act so powerfully on the lead, that in a short time the cistern in which the water was collected became covered with loose carbonate of lead, and the metal could easily be detected in the state of oxide dissolved in the water. In this case, the action was found to depend on the spring being of extraordinary purity, its total saline ingredients being only a 22,000th part. In the other instance, water conveyed half a mile in a lead-pipe, was impregnated exactly in the same way, and with the very same phenomena,—but with the additional circumstance, that, in consequence of the impregnation not having been detected in time, as in the previous case, the disease, *Colica pictonum*, broke out in the house supplied with the water. In this case, the water was by no means pure, as it was found to contain no less than a 4,500th part of saline matter. But there was scarcely any other salt present except muriates, which the author had ascertained in his former researches not to prevent the action of water on lead, unless present in much larger quantity.

He next proceeded to explain in what manner the action of the water was put an end to in both these cases. In similar instances, the only remedy formerly thought of was the substitution of iron-pipes. In the former of the two cases which fell under his notice, the water was left at rest in the pipe for four months, till a firm crust of mixed carbonate and sulphate of lead had crystallized on the lead; after which no farther action took place. In the latter instance, the same end was attained by keeping the pipe full of a solution of phosphate of soda, consisting of a 27,000th of the salt.

The author appended an analysis of the compound formed by the action of distilled water on lead. Guyton-Morveau and others considered it a hydrated oxide; the author himself, in 1829, thought it a neutral carbonate; and, in 1834, Captain Yorke first considered it a hydrated oxide, and eventually concluded from his analyses, that it is an irregular mixture of hydrated oxide and carbonate of lead. The author finds that the product is a hydrated oxide, when the action goes on without the access of carbonic acid; but that, when the action proceeds in the usual way, under exposure to the atmosphere, the product is a crystalline body, of which the primitive form seems to be the regular octahedre, and which is composed of two equivalents of neutral carbonate, united with one equivalent of hydrated oxide ($2\ PbO\ CO^2 + PbO\ Aq$).

He then stated the following to be the general conclusions to be drawn in a practical point of view, from his present and previous inquiries as to the use of lead for conveying water:—

1. Lead-pipes ought not to be used for the purpose of conveying water, at least where the distance is considerable, without a careful chemical examination of the water to be transmitted.

2. The risk of a dangerous impregnation with lead is greatest in the instance of the purest waters.

3. Water, which tarnishes polished lead, when left at rest upon it in a glass vessel for a few hours, cannot be safely transmitted through lead-pipes without certain precautions.

4. Water, which contains less than about an 8000th of salts in solution, cannot be safely conducted in lead-pipes, without certain precautions.

5. Even this proportion will prove insufficient to prevent corrosion, unless a considerable part of the saline matter consist of carbonates and sulphates, especially the former.

6. So large a proportion as a 4000th, probably even a considerably larger proportion, will be insufficient, if the salts in solution be in a great measure muriates.

7. In all cases, even though the composition of the water seems to bring it within the conditions of safety now stated, an attentive examination should be made of the water, after it has been running for a few days through the pipes. For it is not improbable, that other circumstances, besides those hitherto ascertained, may modify the preventive influence of the neutral salts.

8. When the water is judged to be of a kind which is likely to attack lead-pipes, or when it actually flows through them impregnated with lead, a remedy may be found, either in leaving the pipes full of the water, and at rest for three or four months, or by substituting for the water a weak solution of phosphate of soda, in the proportion of about a 25,000th part.

The following Donations were presented to the Society since last Meeting.

Proceedings of the American Philosophical Society. Vol. ii. No. 19. —*By the Society.*

Laws, Regulations, and Annual Report of the Leeds Philosophical Society for 1840–41.—*By the Society.*

Journal of the Asiatic Society of Bengal, 1841. Nos. 112, 113, and 114.— *By the Society.*

Ordnance Survey Maps of England and Wales. Nos. 77, 78, and 87.—*By the Master-General of the Ordnance.*

Karten der Isothermen-Curven auf der Nordl. Hemisphære. Von Wilh. Mahlmann.—*By the Author.*

Comptes Rendus Hebdomadaires des Séances de l'Académie des Sciences. Tome xiii. Nos. 22, 26. Tome xiv. Nos. 1, 2, 3. —*By the Academy.*

Monday, 22d February.

The Right Hon. LORD GREENOCK, V.P., in the Chair.

1. On the Necessity of the Sense of Muscular Action to the full Exercise of the Organs of the Senses. By Sir Charles Bell, K.H.

Referring to the nerves of touch, as distributed to the points of the fingers, the author proceeded to shew that something more was necessary than the exposure of the nerve. For example, the tongue being the most perfect organ of touch, resulting from the fine supply of nerves, is yet incapable of receiving certain impressions;—so that although it could distinguish the finest hair, so fine as not to be felt by the finger, yet if applied to the artery at the wrist, it conveys no sense of pulsation.

Part, therefore, of the organization of the finger consists of a ball of elastic matter, under the nerves, peculiarly suited for receiving pulsation; and being moulded by pressure, by this means conveying the sensation of form. It could not be that the variety in sensation results from the depth to which the impression is conveyed, for the nerves of sensation are on the surface, and the deeper parts do not feel. It must then be the change or moulding of the elastic cushion, consequent on pressure, that is conveyed to the nerve.

After shewing that weight so little different as that of a sovereign and a shilling could be distinguished when placed on the tip of the finger (although no distinction could be made when the pieces of money are laid upon any other part of the surface of the body, however delicate the sensation), he went on to shew the happy combination of the muscular action of the hand and fingers with these *palpi*.

Referring to the sense of muscular action, he gave reasons for the opinion that we had a consciousness of it independent of the sense of touch, or of any of the other senses. He illustrated this by the effect of a species of paralysis, in which the sense of muscular motion was lost, whilst the power of action remained.

Referring to the experiments of Weber and others, by pricking the skin, he stated, that on pricking certain parts of the body with two sharp points at the same moment, there was a sensation of one point only, whilst, on other parts, the sensation of the two points was distinct. Those experimenters drew the conclusion that this capacity of distinguishing the two points resulted from the greater concourse of nervous filaments. He stated, on the contrary, that the capacity of distinguishing the two points, or, in fact, of distinguishing the form of bodies, did not result from the mere sensibility of the part; for example, that the back of the hand was painfully pricked, and yet was not capable of distinguishing the two points from the one. He contended, on the contrary, that the capacity resulted from the junction of sensibility and motion. It was limited to the hand, the tongue, and the lip.

Pricking the cheek with two points gave no distinct sensation, but on approaching the lower lip the points become distinct. And not only the sensibility to *form*, but to *weight* also, results from the combination of muscular motion. Hence the sovereign and the shilling may be distinguished by their weight on being placed on the lip or tongue, though not by any less moveable part.

The author proceeded to consider the motions of the eye, arguing, that to the full exercise of the organ in vision the action of the recti muscles of the eye was necessary, and that a sense of their action was combined with the impression on the retina.

After some instances drawn from the *musci volitantes,* and the experiments of Dr Wells, he stated, that when the impression was permanent on the spot of the retina, the eye being closed, the perception of place was received from the sense of the action of the recti muscles. But if the ball of the eye was moved by the point of the finger, or by the oblique muscles, there was no apparent change in the place of the image. On this subject, he referred to the case of the *nystagmus bulbi,* in which there was an incessant motion of the eyeball, whilst the person saw distinctly, and could perform most minute works with the needle or the pencil.

The fact on which the author placed the greatest stress was the effect of cutting one of the recti muscles. He referred to a late volume of the Phil. Trans. of London, descriptive of a case, in which a lad, blind from birth, had at eighteen years of age the sight of one eye restored. But the eye being distorted towards the nose, the rectus internus muscle was cut across. The consequence of this was, that he no longer saw objects in their right position. They appeared placed to that side on which the muscle had been cut. The author's explanation is, that the tendon of the muscle being cut off from its attachment to the ball of the eye, the muscle contracted, and sensibility to this contraction being associated with the impression on the retina, produced a false perception of the place of objects.

The author concluded by expressing his doubt whether his course of study fitted him to be a competent judge of some of those matters. He could not resist the conclusion, that these required the exercise of a double sense, and an operation of the mind of the nature of comparison, to rouse to consciousness and the knowledge of external existences.

2. Geological Notes on the Alps of Dauphiné. By Professor Forbes. Part 1st. *See Proceedings of the next Meeting.*

The following Gentlemen were duly elected Ordinary Fellows of the Society :—

John Davy, M.D. ;
Robert Nasmyth, Esq., F.R.C., Surg.

The following Donations were presented to the Society since the last Meeting :—

Twelfth Report of the Scarborough Philosophical Society. 1841. —*By the Society.*

Comptes Rendus Hebdomadaires des Séances de l'Académie des Sciences. Tome xiv. Nos. 4, 5, 6, 7.—*By the Academy.*

Report of the Commissioners appointed to consider the steps to be taken for Restoration of the Standards of Weight and Measure.—*By the Commissioners.*

Letter to the Right Honourable George Earl of Aberdeen, on the State of the Schools of Chemistry, in the United Kingdom. By W. Gregory, M.D., F.R.S.E., &c.—*By the Author.*

A faithful Record of the Miraculous Case of Mary Jobson. By W. Reid Clanny, M.D., F.R.S.E.—*By the Author.*

Bien-Etre et Concorde des Classes du Peuple Français. Par Le Baron Charles Dupin.—*By the Author.*

Vie d'un Bienfaiteur du Peuple, A. P. de la Rochefoucauld. Par Le Baron Charles Dupin.—*By the Author.*

Monday, 7th March.

Sir T. M. BRISBANE, Bart., President, in the Chair.

1. On the most recent Disturbance of the Crust of the Earth, in respect to its suggesting a Hypothesis to account for the Origin of Glaciers. By Sir George Mackenzie, Bart.

In this paper the author pointed out that the force, which had protruded granite and other matter among the strata, could not have been the same in point of time with that which broke up the crust of the earth into its present shape; because the matter in veins which we now see exposed to view on the faces of precipices, having been fluid at the time of protrusion, must have run out of the fissures on the rock being raised up. It appeared, therefore, that all matter filling fissures in rocks, had become solid before the most recent cataclysm had taken place. There being no appearance of any matter having been protruded from below at the time of the surface being forced into its present disrupted state, it was assumed that the expanding force which broke the crust, was that of vapourable matter, chiefly steam, confined between the solid crust and the igneous mass in the interior; and also filling up the place of the enormous masses of matter which had been, at a former period, forced up from below, and which we now see in the shape of veins and dykes, and also of those very extensive ranges of country wholly composed of trap beds, admitted to have been submarine lavas. The vapourable matter having increased, and also its expansive force, the crust at length yielded; and the vapourable matter being set free, two consequences were supposed to have followed. First, the vapourable matter must have carried with it a vast amount of heat, so as to cause a considerable refrigeration of the broken crust, and to which its exposure to atmospheric influences, and perhaps also to the rushing of the waters, would contribute. Secondly, supposing the cataclysm to have taken place in the winter season, the extraordinary quantity of vapour forced by expansive impulse into the atmosphere, would speedily arrive at a region whence it would fall back in a frozen state. This would probably have taken place even in the summer season, at least in so far as to load the mountain summits and valleys lifted above the line of perpetual congelation. A change of climate having been effected by the disruption of the crust, the winter influences would, for a long time, have greater power, so that the whole surface within the sphere of a cataclysm might have become covered with ice, until the crust should begin to recover its temperature from the internal source of heat, and the influence of the sun conjointly. The author supposed that, on account of the surface having had, probably, but little elevation previous to the cataclysm, the heat of the crust was so considerable, as, joined to the influence of the sun, to be sufficient to have induced on the temperate zone a climate more nearly allied to that of the tropics, and such as to have admitted of the residence of those animals requiring a comparatively warm climate, and whose remains have given rise to much difficulty in geological speculations. As a subsidiary source of vapour, the author supposed that at the moment of the cataclysm, much water would have been admitted to contact with the interior heated surface. He was not aware, at the time he read his paper, that in a recent work by Charpentier on glaciers, this geologist had appealed to the same thing as the only source of the vapour ; so that the author's hypothesis of the escape of pent up vapour, as the chief source for producing glaciers, still remains his own ; and in conjunction with that of Charpentier, it seems to furnish a theory of glaciers, which, in the present state of discussion respecting glacial action, is a desideratum.

2. Geological Notes on the Alps of Dauphiné. By Professor Forbes. Part 2d.

The district proposed to be described, in so far as it was studied by the author in two journeys in 1839 and 1841, is an out-lyer or appendage to the main Alpine chain, which occupies a considerable portion of the old province of Dauphiné, and the modern departments of the *Hautes Alpes* and *Isére*. It is bounded, roughly, by the rivers Arc and Isére on the north, and by the Durance and the Drac in other directions. Its nucleus is essentially granitic, against which sedimentary deposits of limestone, of different ages, and especially of lias and chalk, repose in highly elevated or contorted strata ; and it not unfrequently happens, that the dislocation of strata has been so great, that the gneiss or granite rocks are superimposed upon the secondary formations.

The granitic mountains of Oisans, which are amongst the highest of the second order of European chains, attain a greater elevation at their culminating point, the Mont Pelvoux, than any of the Alps between Mont Blanc and the Mediterranean. Even Mont Iseran and Monte Viso are surpassed in height by this summit, which measures 13,468 English feet. The ravines by which the chain is intersected have a corresponding depth and ruggedness, so that the *cols*, or passages from one valley to another, are generally covered with perpetual ice and snow, and present, besides, more continuous and inaccessible precipices than are common in any part of Switzerland. The author shortly described several journeys made through the central part of this district, in which it became necessary to cross *cols* of above 10,000 feet in height, from whence alone an intimate knowledge of the structure of these mountains can be obtained.

Guided by the interesting memoir of M. Elie de Beaumont, on the geology of the *Montagnes d'Oisans*, and by the admirable map of Bourcet, he was enabled, in a great many particulars, to verify the observations of the first named distinguished geologist, especially as refers to the phenomena visible at the contact of the calcareous and granitic rocks, which left no doubt on the author's mind that the superposition of the latter to the former is undeniably true. No more can it be doubted, that, as M. E. de Beaumont affirms, we have here evidence of the extensive elevation of previously deposited sedimentary rocks, probably by the appearance from below of the granite itself. Professor Forbes feels some hesitation in admitting, with M. de Beaumont, the *crateriform* nature of this elevation, as indicated by a *quâ-quâ-versal* dip of the stratified rocks round a central point in the neighbourhood of the Mont Pelvoux, and by the radiation of the vallies from that centre. He considers that the observations of the great French geologist, when analyzed, as well as his own, rather point to an anti-clinal axis passing through the point in question, and prolonged in a NN.W. and SS.E. direction; accompanied, however, with various minor lines or centres of dislocation, especially that which elevated the mountain of Grande Rousse to the northward, of which the geology has been ably described by M. Dausse. The interference of this elevation with the previous one (roughly parallel to the torrent of the Veneau), probably produced the excessive disturbance of the strata of lias near La Grave, which have been jostled between the two granite masses.

These views are supported, partly by a consideration of the external *contour* of the group, and partly by direct observations of the bearing and dip of the strata.

The following Donations were presented to the Society since the last Meeting:—

Lexicon Syriacum Chrestomathiæ Kirschianæ denuo editæ accommodatum a Georgio Henrico Bernstein. Part 2.—*By the Author.*

Madras Journal of Literature and Science. Oct. and Dec. 1840. —*By the Madras Lit. Society.*

Letter to the Right Honourable the Chancellor of the Exchequer from J. E. D. Bethune, Esq. on Weights and Measures.—*By the Author.*

Address delivered at the Anniversary Meeting of the Geological Society of London, on the 19th of February 1841; and the announcement of the award of the Wollaston Medal and Donation Fund for the same year. By the Rev. Professor Buckland, D.D. &c.—*By the Author.*

The American Journal of Science and Arts, conducted by Professor Silliman, for January 1842.—*By the Editor.*

Mémoires de la Société Géologique de France. Tome 4, ptie. 2.—*By the Society.*

A Lecture on the employment of the Microscope in Medical Studies. By John Hughes Bennett, M.D.—*By the Author.*

Notes sur le Développement de Nerfs Particuliers à la surface du Cervelet. Par le Docteur Bennett.—*By the Author.*

Monday, 21*st March* 1842.

Dr ABERCROMBIE, V. P., in the Chair.

The following communications were read:—

1. On a New Species of British Grass of the genus Holcus, and Observations on some of the more closely allied species of Grasses found in the Neighbourhood of Edinburgh. By Richard Parnell, M.D., F.R.S.E.

This grass of the genus Holcus was stated to be new to the British Flora, since no mention of it is made in the works either of Sir James Smyth or Sir William Hooker; and as the author was unable to find it noticed in the continental works, he proposed for it the name of *Biaristatus*, as illustrative of the species. Several specimens gathered in the neighbourhood of Edinburgh were exhibited, and the characters by which it differed from *Holcus mollis* and *Holcus lanatus* were pointed out.

Specific Characters.—Both florets awned ; sheaths of leaves smooth ; joints slightly hairy. (See Plate I.)

Description.—The *Holcus biaristatus* grows to the height of two feet or more, but more frequently of about eighteen inches. The root is perennial, fibrous, somewhat creeping. The stem round, smooth, and striated, bearing four or five leaves with smooth sheaths ; the upper sheath crowned with a prominent obtuse ligule. Joints five in number, furnished with a few delicate hairs with their points directed downwards. Leaves flat, broadish, acute, of a light green with whitish rough margins, both surfaces soft and slightly roughish to the touch. *Inflorescence* mostly simple panicled. *Panicle* erect, the rachis and branches hairy. *Spikelets* pendulous, rather large and few. *Calyx* of two glumes nearly of equal lengths, membranous, acute, hairy at the keels ; lower glume the smaller ; upper glume with a prominent green rib on each side. *Florets* two, both awned, of two paleæ, the outer palea of the lowermost floret about the same length as the small glume, pedunculated, of a lanceolate ovate form, smooth and glossy, with two delicate lines or ribs on each side ; keel slightly hairy towards the upper half, the base furnished with a few slender white hairs ; the upper or inner palea membranous, about equal in length to the outer palea, and minutely fringed at the margins. The upper floret sometimes wanting, but when present of about the same size and appearance as the one below, and elevated on a smooth peduncle about one-third the length of the floret. *Awn* rough, arising from a little beneath the apex of the outer palea, about equal in length to the palea, mostly straight, and projecting conspicuously beyond the calyx.

Holcus biaristatus differs from *Holcus mollis* and *Holcus lanatus* in the following respects :—In *Holcus mollis* (see Plate I.) the lowermost floret is of an ovate form, about half the length of the small glume, without a long dorsal awn or lateral ribs, whereas in *Holcus biaristatus* the lowermost floret is ovato lanceolate, about equal in length to the small glume, furnished with a long dorsal awn, and the sides with two lines or ribs.

In *Holcus lanatus* (see Plate II.) the lowermost floret has no dorsal awn, the uppermost floret with a short curved, smooth awn, whereas in *Holcus biaristatus* the lowermost floret has a long dorsal awn, and the uppermost floret has a long straight rough awn.

An unusual variety of *Holcus mollis* was at the same time exhibited, differing only in the spikelets being much smaller and the panicle more compact (see Plate II.). Dr Parnell concluded the paper by making a few remarks on some of the more closely allied species of grasses. Fig. 1, spikelet; fig. 2, florets; fig. 3, glumes.

2. On the Ultimate Secreting Structure of Animals. By John Goodsir, Esq. Communicated by Professor Syme.

After referring to the labours of those anatomists who had verified Malpighi's doctrine of the follicular nature of gland ducts, the author alluded to Purkinje's hypothesis of the secreting function of the nucleated corpuscules of these organs. In a rapid sketch of the results of inquiries since the appearance of Müller's work "De Penitiore Structura Glandularum," and more particularly of the observations of Henle and others on the closed vesicles which are situated at the extremities of certain ducts, Mr Goodsir stated, that no anatomist had hitherto "proved that secretion takes place within the primitive nucleated cell itself, or had pointed out the intimate nature of the changes which go on in a secreting organ during the performance of its function."

Numerous examples were now given of secretions detected in the cavities of nucleated cells of various glands and secreting surfaces. Among these secretions were the ink of the Cephalopoda, and the purple of *Janthina* and *Aplysia*, bile in an extensive series of animals, urine in the mollusk, milk, &c.

The wall is believed by the author to be the part of the cell engaged in the process of secretion. The cavity contains the secreted substance, and the nucleus is the reproductive organ of the cell. A primitive cell engaged in secretion is denominated by the author a primary secreting cell; and each cell of this kind is endowed with its own peculiar property according to the organ in which it is situated. The discovery of the secreting agency of the primitive cell does not remove the principal mystery in which the function has always been involved; but the general fact that the primitive cell is the ultimate secreting structure is of great value in physiology, inasmuch as it connects secretion with growth as phenomena regulated by the same laws; and explains one of the greatest difficulties in the science, viz., why a secretion flows from a free surface only of a secreting membrane,—*the secretion exists only on the free surface enclosed in the ripe cells which constitute that surface.*

The author then proceeded to the consideration of the origin, the development, and the disappearance of the primary secreting cell—a subject which necessarily involved the description of the various minute arrangements of glands and other secreting organs. After describing the changes which occur in the testicle of *Squalus cornubicus* when the organ is in a state of functional activity, and in the liver of *Carcinus mœnas*, it was stated that these were selected as examples of two orders of glands denominated by the author vesicular and follicular.

The changes which occur in the first order consist in the formation and disappearance of closed vesicles or acini.

Each acinus might be, first, a single cell, denominated by the author the primary or *germinal* cell ; or, secondly, of two or more cells enclosed in the primary cell, and produced from its nucleus.

The enclosed cells he denominates the secondary cells of the acinus, and in the cavities of these, between their nuclei and cell-walls, the peculiar secretion of the gland is contained. The primary cell with its included group of cells, each full of secretion, is appended to the extremity or side of one of the terminal ducts, and consequently does not communicate with that duct, a diaphragm formed by a portion of the primary cell-wall stretching across the pedicle. When the secretion in the group of included cells is fully elaborated, the diaphragm dissolves or gives way, the cells burst, and the secretion flows along the ducts, the acinus disappearing and making room for a neighbouring acinus, which has in the mean time been advancing in a similar manner. The whole parenchyma of glands of this order is thus, according to these observations, in a constant state of change,—of development, maturity, and atrophy,—this series of changes being directly proportional to the profuseness of the secretion.

In the second order of glands, the follicular, as exemplified in the liver of *Carcinus mœnas*, the germinal cell or spot, is situated at the blind extremity of the follicle, and the secreting cells, as they advance along the follicle, become distended with their peculiar secretion.

Among other general conclusions deducible from these observations, it appeared that ducts are to be considered as intercellular passages, into which the secretions formed by cells are cast.

Finally, the author inferred from the whole inquiry, 1. That secretion is a function of the nucleated cell, and takes place within it; and, 2. That growth and secretion are identical—the same process under different circumstances.

The following gentlemen were duly elected Ordinary Fellows of the Society:—James Millar, Esq. F.R.C. Surg.; Sir James Forrest, Bart. Lord Provost of Edinburgh; James Stark, M.D., F.R.C. Phys.; John Adie, Esq. Optician.

Monday, 4th April 1842.

Sir T. M. BRISBANE, Bart., President, in the Chair.

1. On the Theoretical Investigation of the Absolute Intensity of Interfering Light. By Professor Kelland.

This Memoir is the prosecution of a subject on which the author had previously touched in a paper which is printed in the seventh volume of the Transactions of the Cambridge Philosophical Society. It has for its object the investigation of the quantity of light which is received on a screen of unlimited dimensions, after passing through a certain aperture, or suffering reflexion at two mirrors. The end for which the investigation is undertaken is to ascertain the *constant* which must be introduced in using Huygens's principle. From the fact, that each vibration at the aperture is multiplied by an area, on finding the effect on the screen, it is evidently requisite that a divisor of two dimensions is required to render the vibration at a point on the screen, similarly expressed with that at the aperture. According to the author's investigation, it appears that the divisor is the product of the length of a wave, and the perpendicular distance between the screen and the aperture. Hence, in reference to such questions, the enunciation of Huygens's principle is as follows:—

The vibration, at a given point, caused by a given wave, is found by taking the front of the wave, dividing it into an indefinite number of small parts, considering the agitation of each of these parts as the origin of a wave whose maximum of vibration, on reaching the point, is equal to the quotient of that at the disturbing point, divided by the product of the length of the wave, and the perpendicular from the disturbed point on the front of the wave.

2. On the Quarantine-Classification of Substances, with a View to the Prevention of Plague. By John Davy, M.D., F.R.S., L. & E.

In this paper, the author, after having noticed the ordinary quarantine classification of substances, into susceptible, non-susceptible, and doubtful, states as the result of his inquiries conducted in Turkey and the Mediterranean, that the distinctions involved in this arrangement have been made in a very unsatisfactory manner, not after careful research and deliberation, but rather during a period of panic, and hastily, in comparatively remote and ignorant times, and by men, for the then state of knowledge, ill qualified for the task, even had they entered upon it with all the calmness and caution that the subject required.

He next enumerates the principal substances constituting the three different classes, and then comments on each of them. His remarks tend to shew that the class of so-called non-susceptible articles, as glass, wood, metal, pottery, &c., since they have not any power of destroying, repelling, or preventing the adhesion of animal matter, ought rather to be considered as susceptible of conveying the matter of plague, in the same way that they are capable of conveying vaccine lymph, *i. e.*, if dried, or in tubes from which air is included; on the contrary, that the substances arranged in the class of so-called susceptible articles, as cotton, wool, fur, feathers, &c., articles abounding in atmospheric air,—the great promoter of the decomposition of putrescible animal matter,—are least entitled to be held fit to preserve and convey the matter of contagion, an inference confirmed by what is known relative to the preservation of animal matter generally, and especially vaccine lymph, and further confirmed by accumulated inference in lazarettos; according to which it would appear that there is no well authenticated instance on record of plague having been produced within the walls of a lazaretto, amongst those persons whose duty it is to examine and expose to the air the so-called susceptible articles imported from the Levant, and especially cotton and silk. In commenting on the subject, the author takes for granted that plague is propagated by a fixed matter of contagion, according to the commonly received views of the contagionists, as it is only on this ground that the quarantine system itself can be supported, or any discussion of the question of the classification of substances be called for. He points out, at the same time, that, preliminary to a searching investigation into the qualities of substances for the practical purposes of quarantine, certain points of the first importance, at present unsettled, ought to be determined *in limine*,—as whether the plague be truly contagious or merely an epidemic disease; whether it is even endemic, independent of contagion; and whether, if arising from local circumstances, it is capable of becoming contagious. He remarks how the quarantine system has been unfortunately established on a basis of suppositions rather than on well ascertained facts, and how, consequently, it is held by all those who have given it their careful attention, to be, as a sanatary system, most unsatisfactory, troublesome, expensive, and insecure. He concludes with the expression of the hope that the time is not far distant when a thorough inquiry into the subject will be demanded preliminary to a revision of the quarantine laws, should the results of that inquiry be that plague is a contagious disease, or capable of becoming so. In the form of an appendix, he relates some particular instances in illustration of his remarks on the quarantine classification of substances, shewing how the distinctions are acted on practically, and how they tend to vitiate the present system of quarantine, supposing it to be otherwise perfect.

3. Results of Experiments on the Specific Heat of Certain Rocks. By M. Regnault of Paris. Communicated by the Secretary.

Professor Forbes observed that, in his communication to the Royal Society on the Conductivity of Soils for Heat, on the 20th December last (see Proceedings, page 343*), he had referred to the separation of the conductivity and specific heat, which are involved in the results of the thermometric experiments on subterranean temperature. In order to eliminate the effect of specific heat, M. Regnault of Paris (well known by his experiments on this subject) undertook, at the request of M. Elie de Beaumont, to ascertain the specific heats of the soils in which the different sets of thermometers are sunk. These are communicated in a letter from M. E. de Beaumont to Professor Forbes, as follows:

	Specific Heat.
Porphyry of the Calton Hill, ...	0.20654
Another experiment, ...	0.20587
Mean. . . .	0.20620

	Specific Heat.
Sand of the Experimental Garden, ...	0.19432
Sandstone of Craigleith Quarry, . . .	0.19257
Another experiment, ...	0.19152
Mean, . . .	0.19205

Some correction would no doubt require to be made for the quantity of moisture contained in the rocks.

4. On the Effect of Snow in apparently increasing the Force of Solar Radiation. By Professor Forbes.

Referring to a communication made by him to the Society on the 1st February 1841 (see Proceedings, page 322), the author reminded the Society that he had then endeavoured to account for certain anomalous facts observed by Dr Richardson, connected with solar radiation in the Polar Regions, by adverting to the intense radiating effect of a covering of snow. The disappearance of this snowy covering in the month of May, the author had observed to be synchronous with the anomalous diminution of solar radiation, ascertained by a blackened thermometer, in the months of June and July, compared with the months of April and May.

Professor Forbes endeavoured to verify his conjecture, by direct experiments on the force of the sun amongst the snowy mountains of Switzerland; and it was so completely borne out, that the limited range of his instrument (Leslie's photometer) was in clear weather always outrun, when it was exposed on a snowy surface; and even when placed upon a dark rock (on the moraine of a glacier), the reflected light from the neighbouring snowy summits was so intense as to give extraordinarily high indications. Owing to the construction of the instruments, he was unable to estimate their readings correctly; but he hopes to make more accurate observations during the ensuing summer. Sir John Herschel's actinometer gave a value of the solar radiation nearly independent of its position upon snow or rock.

The following Donations were presented to the Society since the last Meeting:—

Annuaire de l'Observatoire Royal de Bruxelles. Par A. Quetelet, Directeur de cet Etablissement. 1842.—*By the Author.*

Nouveaux Catalogue des Principales Apparitions d'Etoiles Filantes. Par A. Quetelet.—*By the Author.*

Annuaire de l'Académie Royale des Sciences et Belles Lettres de Bruxelles. Pour l'An. 1842.

Bulletin de l'Académie Royale des Sciences et Belles Lettres de Bruxelles. 1841, Nos. 7–12; et 1842, Nos. 1, 2.—*By the Academy.*

Magnetische und Meteorologische Beobachtungen zu Prag. Herausgegeben von Karl Kriel. Erster Jahrgang.—*By the Author.*

Philosophical Transactions of the Royal Society of London, for the Year 1841. Part 2.—*By the Society.*

Examination Papers of the University of London for 1841.—*By the University.*

Plausible Reasons and Positive Proofs, shewing that no portion of the Devonian System can be of the age of the Old Redstone. By the Rev. D. Williams, A.M., F.G.S.—*By the Author.*

The Reminiscences of an Old Traveller throughout different parts of Europe. By Thomas Brown, Esq.—*By the Author.*

Monday, April 18, 1842.

The Right Hon. Lord GREENOCK, V.P., in the Chair.

The following communications were read:

1. On the Structure, Formation, and Movement of Glaciers; and the probable cause of their former extension and subsequent disappearance. By James Stark, M.D., F.R.S.E.

The author endeavoured to prove, from the recorded facts stated by different writers, that the crystalline particles of which the ice of glaciers is composed do not sensibly enlarge after being consolidated into compact ice; that the crystals have been shewn to be fully and perfectly formed in the course of a few nights in the Polar Regions; and that they have a position perpendicular to the layer of ice which they form,—their length being thus determined by the thickness of that layer.

The author next considered the different forms of stratification met with in glaciers, and stated that the greatest confusion prevailed on this point, different forms of stratification being confounded together. He therefore considered glaciers as composed of—

1. *Horizontal Strata*, or layers lying in the position in which they were first deposited, and only seen in the upper regions of the mountains. He stated that these strata were usually regarded as

marking the additions which the icy mass had annually received, each layer being the accumulated snow of one year; but that, as the Meteorological Tables kept at the Hospice of the Great St Bernard shewed that from 300 to 700 inches of snow fell during the six winter months, it seemed possible that each layer marked the separate storms of snow; or, if they marked the annual accumulations, they apparently proved, what had not previously been suspected, that snow and ice waste nearly as rapidly in the upper as they do in the lower regions.

2. *Vertical and Longitudinal Strata.* The author stated that these strata were always of great tenuity, were more or less perpendicular, but had always a direction parallel with the retaining wall or length of the glacier. Their mode of formation he attributed to the onward movement of the glacier leaving narrow spaces intervening between the sides of the already formed icy mass and the flanks of the valley, which, being filled up with the loose and softened snow lying on the sloping flanks, was, from the falling of the temperature during the night, and from contact with the already formed icy mass, converted into a layer of solid ice. From the thinness of these layers, the author regarded them as marking the additions which had been daily made to the glacier. The author also stated that it would, in all probability, be found that, wherever pillars, pyramids, or needles of ice were met with, this structure would be found present; as the fissures, which always crossed the glacier from side to side, divided it into transverse sections, which, when unequally supported below, would split into smaller fragments in the planes of their stratification, so that each fragment would necessarily assume the form of a vertical prismatic column.

3. *A combination of the Horizontal with the Vertical and Longitudinal Strata.* The author stated that, as the mass composed of the horizontal strata of the upper regions slowly advanced to the lower ones, it received, in the manner above stated, a lateral increase, which, at the same time that it increased its breadth, probably also added to its depth. That, as the glacier continued to advance, the horizontal strata, which lay uppermost, would melt away first, so that at one point they would only be observed in the middle of the glacier, and lower down even completely disappear. He mentioned several facts which seemed to prove his position.

4. *Transverse more or less inclined Strata.* The author stated

that this variety of stratification had not been recognized as a distinct form, but had been confounded with the horizontal stratification. He stated that this form would only be met with when the original structure of the glacier had been broken up and destroyed by some obstructing barrier or other cause. He instanced as the most marked example of this the terminal portion of the Rhone glacier, after it pours into the valley of the Rhone over its rocky barrier or precipice. He described the strata as being formed close to the icy mass on which the icy cataract descends, originally parallel to each other, and with a dip of 70° ; but that, as new layers are formed, and the first formed layers are pushed forwards, they lose their parallelism to each other, and assume angles of dip less and less as they approach the termination of the glacier. This change of dip and of parallelism the author attributed to the forward movement and plasticity of the mass, together with the greater amount of friction below, where the ends of the layers were in contact with the ground, and the constant deprivation of support anteriorly and below, from the continued melting of the ice at these parts, which would give the layers a constant tendency to fall forwards.

The author then proceeded to shew that fissures or crevices in glaciers could not be produced in consequence of the unequal expansion of the ice itself, nor in consequence of the expansion of the air contained within its pores ; but that in every case crevices were produced in censequence of the movement of the glacier over the inclined plane on which it rested.

The author next passed to the second division of his subject, the Movement of Glaciers, and first commented on the Dilatation Theory. He endeavoured to prove that none of the phenomena observed in glaciers could be accounted for by that theory. That a glacier was not retarded in its movement though riddled with crevices; that the supposed dilatation did not alter the form of the walls of these crevices; that it did not close them at their upper extremity nor widen them out below ; that it did not give rise to any convexity of the surface of the glacier ; that the icy mass did not require to touch the rocky walls of the valley through which it passed ; that it could move onwards for miles quite unsupported on its margins ; that during a whole summer, whilst its movement was greatest, it never dilated even the few feet requisite to fill up the spaces intervening between its margin and the rocky walls of the valley ; that it advanced during the heat of the day, and during winter, when it is allowed no dilatation can take place; that it was unlikely water could percolate during the course of one day through a solid mass of ice, more than 100 feet thick, especially when that ice was colder than the freezing point of water; that pools of water (in the Polar Regions) remained unfrozen for whole weeks during the summer whilst their progressive motion was greatest. For these and other reasons, the author arrived at the conclusion "that glaciers do not advance in consequence of a process of dilatation of their icy mass."

The author next enquired into the proofs of the truth of the sliding theory, and stated, that he had satisfied himself that every phenomenon known to occur in glaciers could be explained by it. He brought forwards, as explanatory circumstances, the descent of avalanches;—the descent of trees, along the slide of Alpnach;—the fact proved by the meteorological tables kept at the Hospice of the Great St Bernard, when compared with the descent of Hugi's hut on the Aar Glacier,—that the greater the fall of snow in the upper regions during winter, the greater is the descent of the glaciers during the following summer;—and lastly, the fact that the higher the mountain range (and of course the greater the quantity of ice or snow), the lower was the level to which glaciers descend. He also endeavoured to shew that the glaciers, or icy masses, covering the mountains, and filling their vallies, at no part of their course are frozen to the soil on which they rest; and that the temperature of the soil covered with deep masses of snow or ice, was probably never below 32° Fahrenheit.

The author made a short digression here, to account for the probable cause of the former extension of glaciers, and their subsequent disappearance. He endeavoured to shew, that the scattered boulders, &c. marking the former extension of glaciers, were all over the surface of the older alluvium (diluvium of Buckland) and he hence endeavoured to ascertain at what period that alluvium was formed. After a full examination of the subject, and especially from the examination of the fossil remains found in that alluvium, he arrived at the conclusion, that the waters of the deluge were the cause of the formation of that alluvium; and he accounted for the former extension of glaciers, by the known effect of water, in the act of evaporating, producing cold, especially when acted on by a brisk wind, which was the state of the earth immediately after the deluge. The increased moisture in the atmosphere at this period, he thought, would furnish ample supplies of snow and ice for the purpose, and being first deposited on the elevated peaks, would rapidly spread over all those extended surfaces which glaciers are thought once to have covered. Their subsequent disappearance he accounted for, by supposing that the icy or snowy covering prevented the loss by radiation of the heat received by the earth's crust from the interior of the earth; since this heat, gradually accumulating below, would in time melt the icy masses at their lower extremities faster than they could be supplied from above, and thus reduce them to their present dimensions. He illustrated this view, by mentioning the fact, that the angular boulders, &c. are pretty equally scattered over all the extended surfaces which glaciers are thought formerly to have covered, but are rarely seen to form the dykes or moraines seen at the terminations of glaciers at present in existence; this fact apparently proving that they must have commenced their decay very shortly after their formation.

The author stated several other arguments in favour of the truth of the sliding theory; from all which he inferred, that the movement was not a continuous, but an interrupted process;—that when the melting of the sides of the mass detached it from its attachment to the sides of the valley, and it became undermined below, by the melting of its base, the force of gravity, unresisted by friction, was brought into play, and it made a sudden progressive movement (which might be only an inch or several feet), when it remained at rest, till the same causes produced a renewal of the same result. He shewed, that though many parts of these icy masses were nearly level, all the upper portions, and many of the lower, were lying over such inclined planes, that gravity could exert its full power in their propulsion; and as the whole icy mass was tolerably solid and continuous, the greater movement of one portion was communicated more or less throughout its whole length, and tended to urge forwards and downwards those parts which had less tendency to move onwards of themselves.

The author also endeavoured to account for the advance of one glacier, and the retirement of another along side of it, by supposing that it was caused by the snows being drifted away from the one valley exposed to the blast, and from which the glacier, which was retiring, descended, and being deposited in deep wreaths in the other, which was probably more sheltered, and from which descended the glacier, which was making destructive advances. The increased accumulation of snow, by furnishing a supply greater than the waste caused the one glacier to advance, whilst the other retired, in consequence of the waste at its lower extremity exceeding the supplies from above.

2. On Plague, in relation to the question of its Nature, whether or not a Contagious Disease. By John Davy, M.D., F.R.S.S. L. & E.

The author, after adverting to the methods of investigating the subject, generally and specially, and expressing preference for the latter method; and, after pointing out how desirable it is in inquiries of an obscure nature, to find out *instantiæ crucis*, brings forward and details some facts, which he considers as such, on the question of the contagion of plague.

In June 1841, when there was plague in Egypt, but not in Constantinople or its neighbourhood, which had been free from the disease three entire years, a ship from Alexandria arrived in the Bosphorus, having plague on board. Of the crew and passengers, 18 out of about 96 died during the voyage; and of the remainder, 9 out of 16 who were taken into the Lazaretto. Moreover, certain persons of Constantinople, employed in the duties of the Lazaretto, contracted the disease,—four altogether, of whom three died.

As the disease, with which they were attacked was the same as that imported, and as this was unquestionably plague, and was isolated in the Lazaretto, these instances seem to be demonstrative, that plague can be propagated by contagion, and that therefore (if there be, as it is presumed, a certain consistency and constancy in diseases) it must be considered a contagious disease, although, on many occasions, owing to obscure interfering causes, it may not spread from person to person, and become epidemic.

The author, till he visited the Levant and became acquainted with these cases, was sceptical as to the contagion of plague; and in consequence, before he allowed them to weigh on his mind as evidence, he examined into them most carefully, without being able to find any circumstance connected with them, tending even to raise suspicion of their accuracy as examples of communicated disease.

Among several points of collateral interest connected with these cases, he adverted only to one, viz. the manner in which the disease may have been communicated,—thereon expressing his opinion or conjecture, that in one of the four, it owed its origin to actual contact, and in the other three, to touching substances, which had shortly before been touched by the plague patients, such as glass, metals, or earthenware; and he assigned the reasons which led him to this conclusion.

3. Analysis of Two New Minerals of the Zeolite Family. By Thomas Anderson, M.D. Communicated by Dr Christison.

The minerals in question are the Phakolite of the Bohemian Mittelgebirge, and Caporcianite, a newly discovered mineral, first observed by Dr Savi, in a copper-mine at Caporciani, in the valley of Cæcino. The author found, that in point of composition, Caporcianite belongs to that division of the zeolites, which comprehend Analcine, Ledererite, Potash-harmotome, Chabasie, and Levyn, and whose constitution is represented by the mineralogical formula $r\,S^2+3\,A\,S^2+z\,A\,q$,—z being a variable quantity, r representing the variable monatomic bases, and A & S alumina and silica. The formula for Caporcianite proved to be $r\,S^2+3\,A\,S^2+3\,A\,q$, and the monatomic base consists chiefly of lime. Phakolite belongs to that groupe of zeolites comprising Gigantolite, Harringtonite, Mesotype, Lehuntite, Mezolite, Scolezite, Pyrargillite, and Antrimolite, whose composition is represented by the mineralogical formula $r\,S^3+x\,A\,S+z\,A\,q$,—the equivalents of alumina and silica being variable, as well as water; and the formula for Phakolite is $r\,S^3+2\,A\,S+3\,A\,q$, the monatomic basis being lime, potash, and soda. The numerical proportions of the constituents of the two minerals were as follows:—

	Caporcianite.	Phakolite.
Silica acid,	52.8	45.628
Alumina,	21.7	19.480
Peroxide of iron,	0.1	0.431
Lime,	11.3	13.304
Magnesia,	0.4	0.143
Potassa,	1.1	1.314
Soda,	0.2	1.684
Water,	13.1	17.976
	100.7	99.960

4. Dr Christison exhibited specimens from the Government Superintendent of Tea Culture in Assam, illustrating

the several ages at which the leaves of the Assam and China Tea-plants are used for making the different commercial varieties of black and green tea.

An examination of these specimens seemed to prove, that the leaves of the China tea-plant, cultivated at the same plantation with the tea-plant of Assam, are considerably less, and somewhat thicker, but otherwise so exactly similar, that the two plants may well be mere varieties of the same species,—an opinion now generally adopted by botanists in India. The specimens further illustrated the doctrine deduced from recent investigations in India, that the different kinds of green and black tea are made from the leaves of one species of plant, collected at different periods of their development. The specimens were collected in April 1841. The unexpanded shoots and very young leaves are marked as yielding Pekoe, a black tea, and Young Hyson, a green tea, by different modes of preparation. The fully-expanded, but still young leaves, are stated to produce Pouchong, Souchong, and Campoi, among the black teas, and Imperial, Gunpowder, and Hyson, among the green teas. Older and firmer leaves produce Congo, a black tea, and Twangkay and Hyson-skins, two of the green teas; and the oldest and coarsest of the leaves produce Bohea, the lowest in quality of the black teas.

The following Donations were presented to the Society since the last Meeting :—

Proceedings of the Royal Society. No. 52.—*By the Society.*

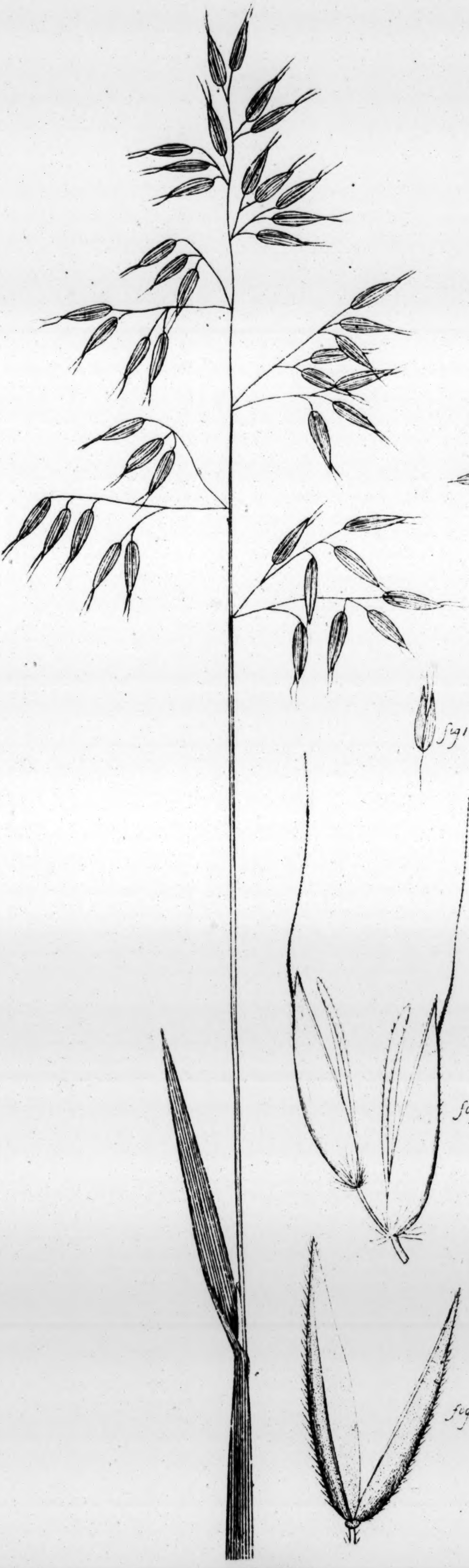
Theoretical Investigations on the Motions of Glaciers. By W. Hopkins, F.R.S.—*By the Author.*

Comptes Rendus Hebdomadaires des Séances de l' Académie des Sciences. Tome xiv. Nos. 8–12 —*By the Academy.*

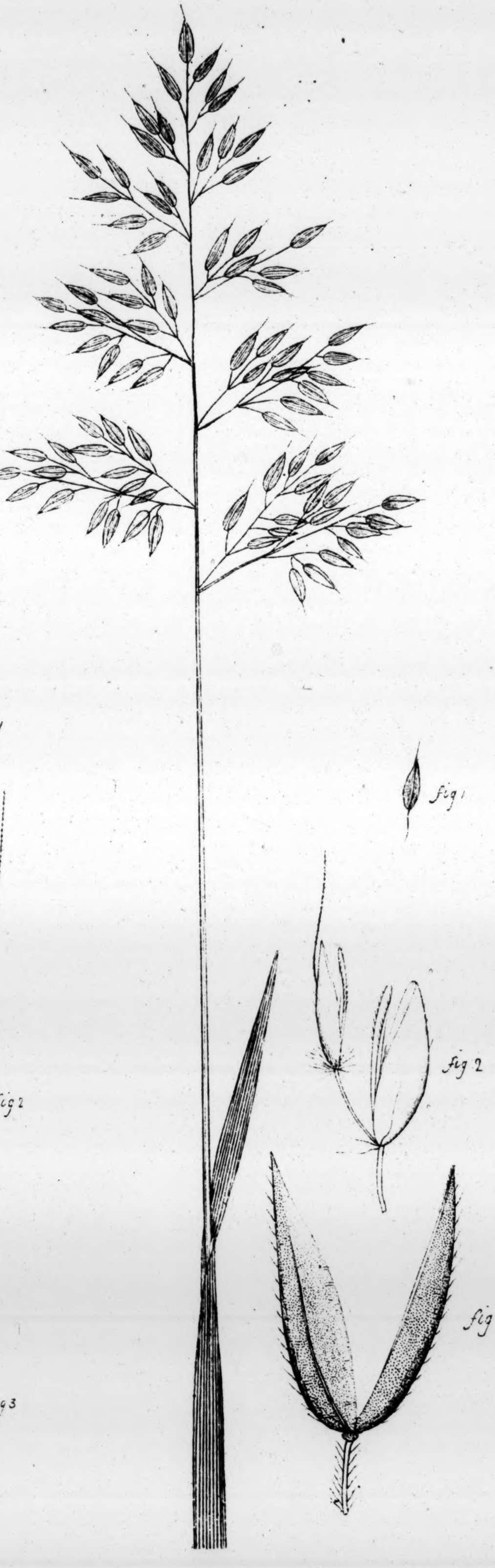
Journal of the Asiatic Society of Bengal. 1841. No. 116.—*By the Society.*

Elements of Agricultural Chemistry and Geology. By James F. W. Johnston, F.R.S.—*By the Author.*

PLATE I



Holcus biaristatus [redacted]

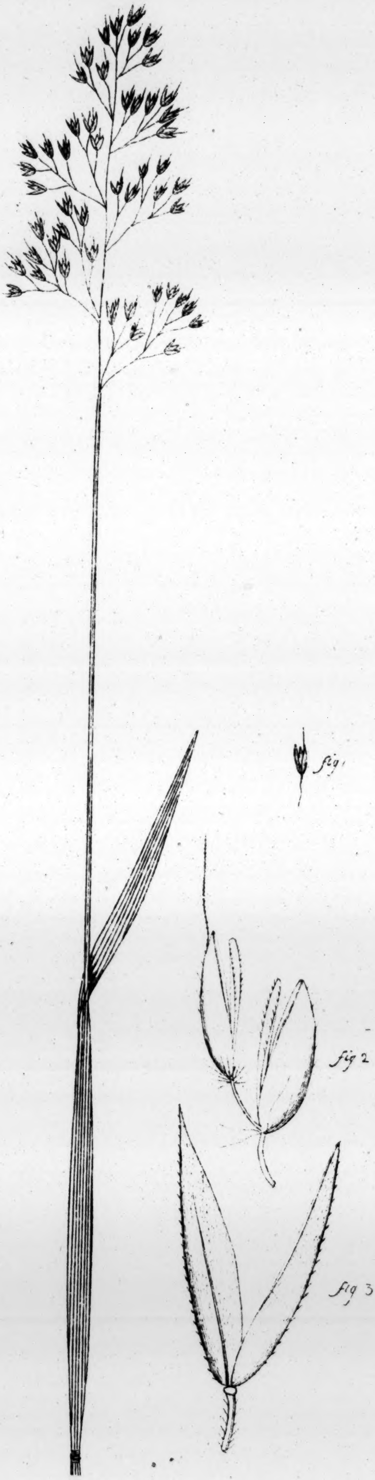


Holcus mollis

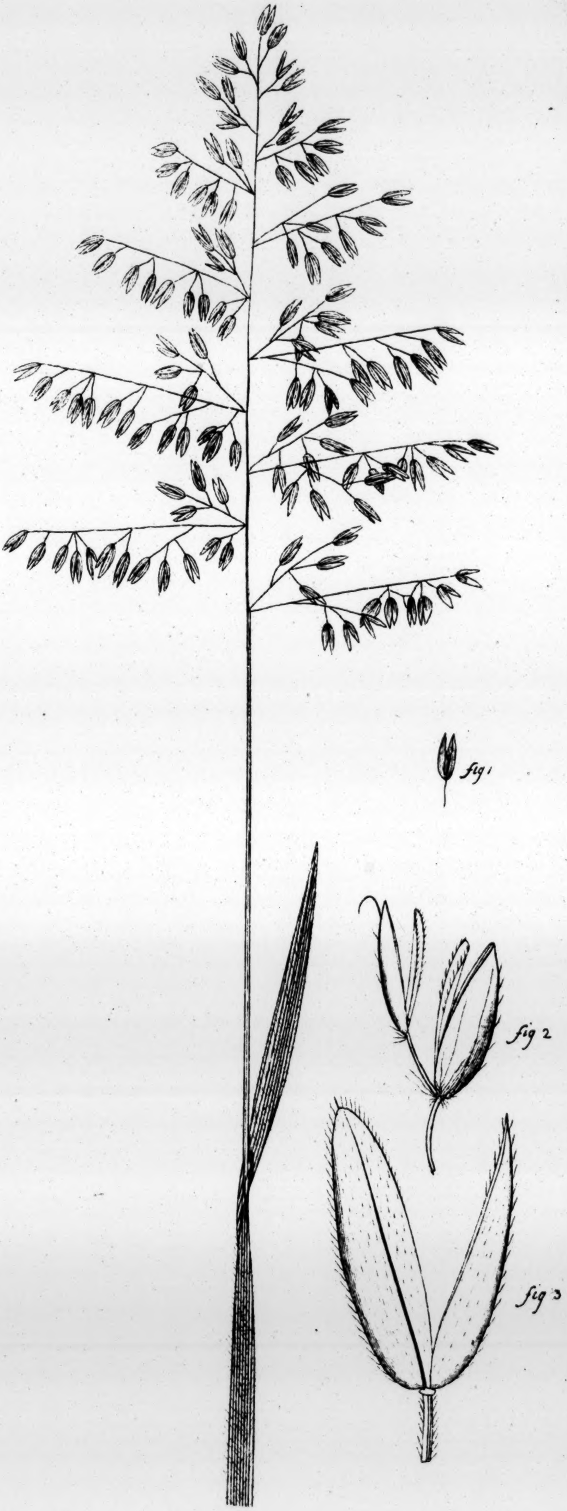
R. Parnell M.D. delt et sculpt.

Printed by J. Gellatly.

PLATE II



Holcus mollis
(Variety)



Holcus lanatus

R. Parnell M.D. delt et sculpt.

Printed by J. Gellatly.